Statistical Tales: Bringing in Reflexivity to Make Sense of Quantitative Data

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Abstract
Reflexivity has long become part of qualitative researcher's arsenal for validity and credibility claims. However, very few quantitative researchers take the time to look back at their research process and ponder over the cost of deriving knowledge from statistical models – what has been omitted, polished, ignored or not taken into account. In this chapter I will try to bring reflexivity into my own quantitative research of young people's environmental behavior by reflecting over what knowledge I have produced so far and why. Having worked with five waves of longitudinal data for two different age cohorts, I lived through several 'existential' crises failing to comprehend the stories that the data was telling me and failing to 'impose' my theoretical stories on it. It has challenged me to unravel conventions and granted assumptions of media studies as a discipline, reflect upon data's temporal and spatial components, the subjective position of the researcher, the limits and the meaningfulness of generalizations, and the role of interpretations in statistical analysis. My personal research journey serves as a helpful background for a discussion of difficulties working with longitudinal quantitative data.

Keywords Reflexivity, Quantitative Methods, Statistics, Positivism

1. Introduction

Despite general acceptance that every research in its core is political and "*knowledge cannot be separated from the knower*" (Steedman, 1991), quantitative research and its objective stance of the inquirer gives an (erroneous?) impression that it is possible to produce neutral and value-free information about a phenomenon (Kincheloe and Tobin, 2009). What I have experienced working with the data, however, was far from a straightforward questions-and-answers session. Although quantitative studies are rooted in positivist philosophy (assuming that there is an objective reality that can be studied) and methodology (the objective reality is represented by variables that are used to draw probabilistic conclusions), it is rarely truly positivist in practice with its rigorous deductive strategy. As Greiffenhagen, Mair and Sharrock (2011: 103) put it, "*models do not build themselves any more than they interpret themselves* [...] *choices are still to be made, and these are frequently based on intuitions, hunches and ideas of what is needed that have not yet been fully rationalized*". Having gone through numerous trials and errors, I have found it necessary to bring in reflexivity to make sense of what I have learned so far and how I have learned it.

Reflexivity has become a trademark of quality in qualitative research. It shifted the center of gravity from mere interpretation of empirical material to the *interpretation of interpretation*, turning attention to the persona of the researcher and all the perceptual, cognitive, theoretical, linguistic, political and cultural circumstances that impregnate the interpretations (Alvesson and Sköldberg, 2009). Much fewer advocates of reflexivity can be found in the quantitative camp. The main debate takes place in sociology and focuses on marrying statistics and interpretivism (Babones, 2016; Gorard, 2006; Johnson and Onwuegbuzie, 2004; Ryan and Golden, 2006) while the debates about the mathematical inadequacies of the way statistics is applied in social research span across the disciplines (Carver, 1978; Gigerenzer and Marewski, 2015; Gorard, 2006; Wright, 2003).

A pragmatist mixed method has been discussed as a solution that bridges the opposites and plays on the strengths of the different methods (Johnson and Onwuegbuzie, 2004). Although it offers quantitative and qualitative researchers an opportunity to bury the hatchet, it does not help to solve some epistemological issues those methods are often criticized for. With no ambition to find the panacea for those issues, the purpose of this chapter is to share my insights of what it means to work with quantitative data as a PhD researcher, highlighting the pitfalls of this road and humbly contribute to the debate on relevance of reflexivity in quantitative research tradition. By writing this text, I am also seeking answers to why it has been so extremely difficult to produce scientific knowledge both rigorous and relevant. Therefore it is necessary to question both what stands behind procedural ritualism of the method and what is considered as knowledge within quantitative

paradigm. These are epistemological reflections on what shaped my research journey as it unfolded. To look upon my research process I will employ Bourdieu's two main *objects* in reflexive social research: the need to test one's own position and perspective as a researcher and the need to question the very foundations of the method (Webb, Schirato and Danaher, 2002).

2. My personal bias and bias of the field

Being a quantitative researcher at heart, I have not been a blank slate but I have brought with me a passion for precision and reliable knowledge (in a way that apple will always fall to the ground) into my PhD project. Although all method books teach to choose a method that fit your research question, as it often happens in research reality, the choice of method preceded the questions. I have joined an interdisciplinary research group that worked with longitudinal quantitative data collected for 5 different age cohorts (from 13 to 26 at the first year of data collection) with samples that varied between 600 and 1000 respondents during 6 years (from 2010 to 2015) in Örebro, Sweden. My own project is about adolescents in Sweden, their environmental engagement and the role of communication (mediated and interpersonal) in their willingness to act in an environmentally conscious way.

Longitudinal survey data and a question about the media's influence have right away put me in an uncomfortable box of media effects – a tradition that has been heavily criticized and sentenced to scientific oblivion, but is nonetheless alive (Bryant and Oliver, 2009). In its earlier years this research tradition focused on political information and included studies on propaganda (Lasswell, 1927) and media's influence on voting behavior (Lazarsfeld, Berelson and Gaudet, 1944) but later included other aspect of people's worldviews. Therefore, an assumption that media influence environmental behavior/values is taken for granted as it was created by comparison (with voting behavior). Pierre Bourdieu (1991), however, makes a clear distinction between simple *resemblance* and *analogy* in contrasting a scientific object by the comparative approach: the former grasp only the external similarities, the latter apprehend the hidden principles of reality. "Media effects on environmental behavior" as a scientific object draws more on simple resemblance than analogy, as the mechanisms of this influence are very different: there are no mass-scaled political campaigns targeting people's environmental beliefs, as the most obvious example. In this case, the use of traditional media effects theories may obscure the scientific object of my research. And when "retranslating" the data that was not collected in relation to my problematic, I run a risk of comparing incomparable and failing to identify the comparable. Therefore, reflexive awareness of what kind of knowledge – and consequently, what kind of reality (Law, 2004) – I produce is needed every step of the way.

The dominating preferences of the field cannot help but influence one's research too. As young people have been often labeled "constant contact generation" (Clark, 2005) or "digital natives" (Prensky, 2001) among other names, there is invisible pressure to study the potential online media effects if one as a PhD student wants to strategically position him/herself in the field. As it seemed to me that to focus on traditional media would mean to marginalize myself in the field, I was compelled to keep online media experiences present in my models.

3. The ghost of induction

To explore the role of the mediated and interpersonal communications in adolescents' environmental engagement I used the data from two different cohorts (13 and 16 years olds at the time of first data collection stage) collected over a span of five years. It is important to mention that I had no part in designing the questionnaire or meeting young people to distribute them. Neither did I have any prior personal experience with Swedish teenagers and their reality. In theory it should not matter, as the knower and the known are considered independent within the positivist paradigm to ensure the objectivity of knowledge. In reality, I had to rely on my understanding of young people that was informed by predominantly American previous studies, and I found no evidence for my expectations in the data. I decided to "get to know" my data and I adopted a more explorative, and therefore inductive, approach. The descriptive statistic was telling unexpected stories that inspired further inquiries. Eventually I hit dead-ends on many of the chosen paths exploring the relationship between skepticism, environmental attitudes and media consumption. Two things were the most problematic. One, the test results did not hold up for both cohorts or for different point in times. And two, I had no good theoretical model to go with it and explain the discrepancies. These two aspects are, in fact, two sides of the same coin – deduction.

The deductive analytical strategy, which underpins quantitative research, requires a rather high degree of prior theorizing. There can be no observations that do not involve hypotheses logically deduced from an existing theory or previous studies. Thus, the whole inductive endeavor of mine was doomed from the start, as regardless of how the actual research process goes at the final stage of presenting my research, I will have to write up my results in a deductive manner. Such a "repackaging" of inductive findings requires cutting the loose ends that do not fit perfectly. Besides a significant loss of knowledge, this practice is considered unscientific. Called "*fishing expeditions*" (Gigerenzer and Marewski, 2015) or HARKing ("Hypothesizing After the Results are Known" – see Kerr, 1998), it is criticized for testing hypotheses with the same data from which they derived, rendering it to

be not a proper science. It is hard to disagree with such evaluation, yet it leaves researchers with a very limited tool kit. Deductive logic does deliver robust results in certain cases, however it does not discover more than what is already known in its premises. And as my premises were mainly supported by Anglo-American research, they fell apart when applied to a Swedish context. Consequently, my only contribution would be to say that things don't work in the same way in Sweden. This knowledge may be robust but hardly relevant to anyone.

Most philosophers of science distinguish between a context of discovery and a context of justification: the former tells how a particular piece of knowledge came to exist, the latter explains its content and the reason for accepting it (Feyerabend, 1987: 110). The quantitative scholarly community only regards validation as a genuinely scientific practice completely ignoring the epistemological hierarchy of scientific acts "which subordinates validation to construction and construction to the break from self-evident appearances" (Bourdieu et al., 1991: 11).

Such epistemological position has a strong implication on the definition of theory. The positivist understanding of theory limits its function to "representing a set of experimental laws as fully, as simply, and as exactly as possible" (Bourdieu et al., 1991: 29). Thus, the main criterion to decide if to pursue an idea was its potential to be formulated in a general unifying law that can be applied regardless of circumstances. Contrary to qualitative research, the power of generalizations rules in quantitative social research. In my case I was looking for patterns across ages and across time but I could not find them – my findings were inconsistent across cohorts or across several waves of data within the same cohort. It is totally plausible that there are no such patterns. In this case to make a contribution to the field with my findings I would need to theoretically argue why a 13 year old differs from a 16 year old in terms of media influence or environmental values. In other words, absence of common features should represent theoretically supported regularity too. To support theoretically such a claim, one needs to assume that each cohort is a homogenous group with a comparable level of maturity that can be juxtaposed with another homogenous group. And by all means, that makes no sense as the only reason why we talk about homogenous group of 13 year olds is because the school system puts them together by their biological age and not at all by their mental development. Thus, in the absence of regularities across time and ages, it is difficult to argue why this knowledge about specific group of Swedish adolescents at this point in time matters.

This conception of social science strongly relies on methodology of natural sciences: society life complies with certain underlying laws that need to be uncovered. In many cases, though, social research outdoes the demands that are attributed to natural science. Scrutinizing laws of physics (an ideal model for positivism), Nancy Cartwright (1983) argued that physical fundamental laws are hardly ever true (unlike phenomenological laws) as they are "*abstract formulae which describe*

no particular circumstances" (p. 11). Context becomes of outmost importance as regularities in nature can only be observed when the circumstances are similar or right. In a similar vein, Stephen Toulmin (1953) compared theories with descriptions of rules. For every rule, we define its domain – all cases for which the rule is valid – and its area of application – the cases for which the rules hold. Consequently, the right question about scientific theories should not be "is it true or false" but "when does it apply". Consequently, to dig myself out of the problem of no law-like theoretically supported results, I turned to the literature and previous studies in search of a good model to pose the question "under what circumstances does it apply?".

4. The tyranny of models

Environmental communication is an established sub-field with a dedicated journal and a group of researchers calling it home. However, theories that are routinely used to produce knowledge within the field are hardly unique or case specific. The studies mainly draw on the established communication models, and political communication models in particular (e.g. Östman, 2014; Zhao, 2009). Thus, if deductive strategy is the only legitimate option for a quantitative researcher, by default the environmental communication will be treated as a case study within communication. Consequently, what can be answered is if it is governed by the same or different logic (a question "by what logic?" cannot be answered deductively). And that is what I tried to find out.

I was interested in mechanisms underlying the media influence on adolescents. Political communication research suggested political information from news did not have a direct influence on people's partisan or voting preferences. However, it often became a food for thought and discussion with others, which later translated into voting preferences or other types of political engagement. This model was dubbed "communication mediation model" (McLeod et al., 2001). It gained popularity and general acceptance and was later specified in "citizen communication mediation model" (Shah, Cho, Eveland, and Kwak, 2005) and extended to "campaign communication mediation model" (Shah et al., 2007) by the same group of researchers. It was also already tested and seemed to work for young people's pro-environmental behavior. However, by drawing parallels with political communication research I found it a bit strange that it worked for young people with all kind of beliefs. Employing mediated moderation analysis, I tested if that was true for both climate change skeptics and "believers". And as I suspected, it did not for skeptics. Their behavior did not change no matter how much news they consumed and discussed with parents. That knowledge totally escaped previous application of the theoretical model. Nor was that knowledge statistically important: when I compared statistical

parameters of how well the two models fit the data, my model did not show much improvement. And why would it show if skeptics are just a small group among vastly environmentally friendly Swedish youth? Thus, although statistically my model has not improved much, I find this difference scientifically meaningful.

"*Models are stories and are not real life*", argues Elliott (1999), as the vocabulary used to discuss them – how well they fit the data – points out that they are not the same as the data. Nor are they the same as theories. The argument that models take a mediator's role between theories and empirical data was formulated both for natural and social sciences (Morgan and Morrison, 1999) under "semantic conception of science". It means that theories are compared with models, and models are compared with the data. There are also unavoidable two-way adjustments at both stages. As Cartwright (1983) puts it, "*approximation and adjustments are required whenever theory treats reality*" (p. 13). And I can add that a great deal of judgment is required whenever model treats the data.

Any statistical model is full of choices. When translating communication mediation thesis into the language of statistical modeling I was presented with different variable options. The model claims that information from news, when discussed with people, turns into action. Working with teenagers, I had to decide how to define people – parents, peers, teachers, or altogether? Putting too many aspects of adolescents' social life in one melting pot is rarely a good idea, so I went with my theoretically informed judgment, choosing talks about environmental issues with parents (e.g. Mead et al., 2012). The same can be said about operationalization of news media use (newspaper, TV, radio or altogether? Should I add online news consumption here or treat it as a separate phenomenon?) and the outcome variable of pro-environmental behavior. Thus, a statistical model becomes a mere approximation of theoretical model, as operationalization choices need to be made in every particular case of application. In the next step, when data is forced into predefined theoretical frameworks expressed through always-approximate statistical models, a lot of it does not fit, but sometimes it is still enough for probability p-values – an index of the weight of evidence against a null hypothesis of mere chance – to be acceptable. People may be different, but communication mediation model seemed to work for prevailing majority, thus swallowing the skeptics (and maybe more smaller groups).

Last but not least, the model did not work for both cohorts and for every year. And just when this knowledge was about to be rendered unscientific, I realized that the external context was the key. The mediated moderation model, that I proposed, only worked for an election year, thus proving that no general pattern could or should be found here. And at this point I changed the course once again and turned from variable-based statistics that looks for patterns between different characteristics to a person-based approach that focuses on patterns among people.

5. P-values as decision makers

During my inductive stage of research, I noticed that scientific skepticism was not the only marker for lack of behavioral commitments. Some young people expressed no doubt about the severity of climatic changes but they did not consider this fact as important for them. Or, even more puzzling case, they think it is important to protect environment but they do not believe in man-made climate change. The studies on climate change skeptics mainly focus on factors that contribute to it to understand how we can design communication in a way so that it helps change their minds (e.g. Ojala, 2015). Some even suggested to consider environmental communication a crisis discipline, thus legitimizing this moral stance and predefining which research questions should take priority (e.g. Cox, 2007). The critique of a moral stance may sound positivistic, but its implications on the breadth of research inquiries are of greater concern. When skeptics are seen as an obstacle to mitigating policies, no one asks what good reasons they have to take this position and what it says about the society as a whole.

Thus, drawing on post-political perspective (Swyngedouw, 2013) and employing cluster analysis, I have identified four different types of people that fall on the spectrum from believers to ultimate skeptics. The types came forward for both cohorts on 4 waves of data. They represented a rock of stability – a dream result when working with statistics. While discussing my types with colleagues, I often heard a tentative objection that they could not identify themselves with any of those types. Of course, it made me think and think again if I should reconsider the "ingredients" in my typology. Acknowledging that often what is individual and idiosyncratic is sacrificed for the sake of finding commonalities, I find this critique informed by positivist logic. The underlying assumption in it is that any quantitative research should be widely generalizable even when there are no statistical or common sense reasons to do so. The types of skeptics found among 13-18 year old Swedish teenagers cannot necessarily (and should not) be applicable to youth outside Sweden, not to mention adults. To make a concept applicable or common is to make it more general and therefore empty. The contrast is similar to the differences between moral principles laid out by Aristotle in the *Nicomachean Ethics*: "*Among statements about conduct those which are general apply more widely, but those which are particular are more genuine*" (Aristotle, 1954:29). Thus, I pursued my inquiry into the role of communication and its potential influence on the behavior for the four types.

To understand the role that communication played in forming skeptical attitudes, I first needed to understand if those attitudes were stable over time. Using Exacon analysis (Bergman and Magnusson, 1997), I found out that the attitudes were quite stable from the age of 13 to 18 years old: skeptics remained skeptics and "believers" kept doing their thing. No, it did not mean that not a single per-

son changed his/her mind but those people simply did not represent a statistically significant trend. Such result seemingly closed any further inquiry into the role of communication in skeptical attitudes. Besides, it tentatively allowed a wider conclusion that if the parents, educators or politicians want to influence young people's beliefs they should direct their effort at much younger cohort. The data showed that at the age of 13 their general attitudes are formed and will remain so. However, this conclusion left behind a handful of those individuals who radically changed their views in the midst of adolescent years. Are the influences behind their decision not worth knowing because they do not form a statistically significant trend? What if their cases represent exactly the right circumstances that explain why it happens so rarely? It is a valid question and plausible assumption but something that statistical analysis tend not to bother with.

The criterion for not pursuing any inquiry into "volatile" cases was *p*-values that lied way above the accepted threshold. The person who proposed to accept results at 5% probability of explanation by chance, Ronald Fisher, also warned that "*no scientific worker has a fixed level of significance at which from year to year, and in all circumstances, he rejects hypotheses; he rather gives his mind to each particular case in the light of his evidence and his ideas*" (Fisher, 1956: 42). Yet, exactly the opposite has become a standard and the only game in town among quantitative researchers and publishers (Gigerenzer and Marewski, 2015). In pursuit of objectivity, social scientists have created a universal method of inference, which, according to Gigerenzer and Marewski (2015), became used for mechanizing scientists' inferences rather than for modeling how nature works. Probability theory with infamous *p*-values have delivered a simple promise to replace the subjectivity of experimenters' judgments with an automatic method (Gigerenzer and Marewski, 2015). And if bigger research disciplines such as psychology or sociology, that have been heavily influenced by quantitative methodology, have opened up for alternative voices arguing for measuring effects sizes, providing confidence intervals or adopting Bayesian statistics (Carver, 1978; Gigerenzer and Marewski, 2015; Wright, 2003), to my knowledge, the field of media and communication takes what is the most convenient (and thus automatic) and do not even enter a methodological conversation. Neither are doctoral students taught to think differently or question the practice in which statistical software makes a decision about importance of the differences presented by data.

Gigerenzer and Marewski (2015) brought out the argument that general acceptance of *p*-values fundamentally changed theorizing both in natural and social sciences, making the inference from a sample to population the most crucial part of research. I see their point but I will also argue that it has not done it alone. The human tendency of privileging theoretical ways of knowing all over the other ones (Arendt, 1958) manifested in deductive thinking and persistent positivist practices

contributed to a smooth adoption of such understanding of objectivity. Using an automatic method without reflection on what it can and cannot say does not bring us closer to understand the social world. In the words of Hannah Arendt (1958: 266), "*mathematics succeeded in reducing and translating all that man is not into patterns which are identical with human mental structures*" and helped to handle the multitude of the concrete by create order out of mere disorder.

6. Where to from here?

I must admit that even after writing this text I am still a quantitative researcher at heart. However, these reflections have brought to light some moments where I myself used statistics as an automatic method or I have not taken into account what the chosen theories did to the scientific objects or objects they made. Numerous observations that have not become part of this text will change and inform my future use of the method.

As it happens when learning or teaching statistical techniques, one can forget for a moment that the method does not free the researcher from a constant epistemological vigilance and should not be used as a "*scientific alibi for blind submission to technical instruments*" (Bourdieu et al., 1991: 10). There is nothing irreparably wrong with the method, but its applications and accepted standards can be often questioned. And here I stand with those who call for interpretative or judgment-based quantitative research (Babones, 2016; Gorard, 2006) that brings the researcher back in the decision-making process. I also argue that one method cannot do it all and does not have to. As use of different measuring tool will always tell us something different about the reality (Barad, 2007; Law, 2004), it is more important to define what a method can do for you.

Being objective and aspiring for objectivity in research might just be mutually exclusive things. If being objective means being distanced and disengaged from the material relying on significance number to establish worthiness of the findings, then it leads to a very superficial understanding of the reality. Lacking personal experience with Swedish adolescents, I was haunted by a feeling of being an alien trying to make sense of the life of Earthlings and always missing something important. Parker (1999: 85) suggested what at first glance seems counterintuitive: "*to put subjectivity as the heart of research may actually, paradoxically, bring us closer to objectivity than most traditional research which prizes itself on being objective*" as it moves us towards a more complete and inclusive account of the reality. Therefore, quantitative research cannot do without reflexivity, as it is not immune to errors in judgment. The concrete reality "*always remains equally individual, equally undeducible from laws*" (Weber, 1949 in Bourdieu et al., 1991: 11).

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Biography

Yuliya Lakew is a PhD Candidate in Media and Communication Studies at Örebro University, Sweden. In her dissertation she explores the role of the media in the development of environmental concern and pro-environmental behavior among Swedish adolescents. The research aims to analyze the interplay of different communication flows - from parents, peers, school, and media - and the conditions under which communication facilitates youth's engagement with environmental issues. She does mostly quantitative research and teaches quantitative methods to undergraduate students.

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